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# *Reply to Mr. Stone's Paper on Screw Errors as affecting the N.P.D. of the Cape Catalogue for* 1880. By David Gill, LL.D., F.R.S., Her Majesty's Astronomer at the Cape of Good Hope.

In the following paper it will be convenient to take up the points raised by Mr. Stone nearly in the order in which he has stated them.

A considerable portion of my paper, which originated the present discussion, was devoted to a proof that errors of considerable magnitude were produced by wear of the screws and not by constraint, as argued by Mr. Stone.

Mr. Stone appears now to have given up that theory, as he does not support it by any further argument; and whilst admitting that wear may take place, he denies that such wear is sufficient to lead to serious errors in the resulting N.P.D.'s.

Here it will be well to define what are to be considered "serious errors" in this discussion.

A Star Catalogue may be judged by various standards.

1st. Merely as a Zone Catalogue, in which the differences of N.P.D. from standard stars are pretty accurately observed, the adopted places of the standard stars being the reference points of each zone. Or,

2nd. The N.P.D.'s may be considered fundamental, that is proper, without the application of any corrections, for discussions of the law and constant of refraction, the determination of precession and the solar motion in space, and for refined investigations on proper motion.

In the former case an error of $0''\cdot5$, due to accidental screw error, cannot be considered "serious." But in the latter case, if even a much smaller error than $0''\cdot5$ is systematically applicable to the observations of a long successive period, then we must regard such error as "serious," because the Cape (1880) Catalogue having been observed in zones of 10° in Declination, all the results of large adjacent portions of the Catalogue will, in such case, be affected systematically by that common error.

From Mr. Stone's expression (p. 140) of the "hope that Mr. Gill will be better advised than to apply empirical corrections to the Cape (1880) Catalogue," it must be understood that Mr. Stone regards the N.P.D.'s of his Catalogue as fundamental, and consequently that criticism belonging to the latter of the two classes above indicated is legitimately applicable to them.

Passing over, for the moment, all questions as to the origin of the screw errors, and appealing directly to facts, as Mr. Stone desires, the matter in discussion is fortunately reduced to a very simple one by the admission of Mr. Stone that "the curve which Mr. Gill has given (representing the errors of the screws) is the

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reproduction of one which I (Mr. Stone) constructed from these observations in 1877."

I have never seen the curve to which Mr. Stone refers; but it is at least satisfactory to find that, from the same observations, we have arrived precisely at the same curve.

That curve gives the figures in Table V., p. 86, of my original paper, and we have therefore only to deal with precise figures about the accuracy of which there is no question.

Any given observation made about the epoch 1877 can thus be rigorously corrected for screw error from the data of Table V., provided that we know the original readings of the screws.

1st. In the observation of the star.
2nd. In determining the runs.
3rd. In the Nadir determination.

Unless, therefore, Mr. Stone can point to some error in the method of computation employed (described at p. 87) he must admit the accuracy of my Tables VII. and VIII.

As Mr. Stone instructed his assistants not to make observations below $5^{\mathrm{r}}{\cdot}0$ (Mr. Stone's negative readings), it will be assumed in the following discussion that no such readings have been made, although, as a matter of fact, not a few of such exist. But apart from these readings we must suppose that the other readings fall with equal frequency on any part of the screw from $5^{\mathrm{r}}{\cdot}0$ to $10^{\mathrm{r}}{\cdot}0$, for it is impossible to restrict the supposed range within narrower limits.

Thus, if

$r_o$ is the accidental mean error of observation,

$r_s$ is the error due to the screw.

Then the mean error of the Catalogue place of any star is

$$r_o = \sqrt{\frac{r_o^{\,2}}{n} + r_s^{\,2}}$$

where $n$ is the number of observations.

This expression would not be true if the star was observed over a considerable range of the Declination micrometer screw, because in that case different readings of the Circle microscopes would have been employed in each observation, and thus there might have been a tendency to eliminate to some extent the errors of these microscopes. But it was the practice of the observers to set the micrometer screw, for periods of many months together, always at or very near the same reading (within a very few seconds of arc), and to place the horizontal wire on the image of the star as it entered the field before clamping in N.P.D. The reason was doubtless to render the observations as nearly as possible independent of the adopted value of the Declination micrometer screw, but it has the effect of bringing the whole errors of the screws at the particular reading into the mean Catalogue place of the star.

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If the reading for a particular star fell between $6^{r}\cdot0$ and $9^{r}\cdot3$ the error due to the Circle micrometer screws (apart from the systematic error of run and Nadir point) never exceeds $\pm0''\cdot1$, *but one-third of the observations must of necessity fall above or below these limits, when the accidental error so introduced ranges from* $0''\cdot1$ *to* $0''\cdot95$. It is surely important to correct such errors, when one has access to the original observations, if a refined investigation of the proper motion of a particular star is being made.

Much as such errors are to be regretted in the original results, they do not affect the Catalogue systematically.

That is to say, if, for example, we compare a number of stars as observed in Europe and at the Cape, for discussion of the law and constants of refraction, such accidental errors, due to the Circle screws, might be supposed to be practically eliminated in the mean of a large number of stars.

But the practice of observing the Nadir for long periods at the same reading of the Declination micrometer, coupled with the errors of the screws, has led to systematic errors extending over large contiguous portions of the Catalogue.

From January 1871, when the work of the Catalogue was commenced, till 1872, May 13, the whole of the Nadir readings (except a few negative readings) are made between $5^{r}\cdot0$ and $5^{r}\cdot5$, for the lower reading of the Circle microscopes, the mean reading, as well as the great majority of the readings being at $5^{r}\cdot3$. During that period the practice followed by all the observers was to set the telescope-micrometer to $10^{r}\cdot0$, and then to clamp in N.P.D., when the direct and reflex images of the horizontal wire were nearly in coincidence. On 1872, May 13, a change suddenly occurs in the Nadir reading, produced by a change in the drum, by which the number of whole revolutions is read off. That is to say, on 1872, May 13, the same nominal reading of the counter drum corresponds with a portion of the screw differing by two revolutions from that which it formerly did. This change seems to have been produced by accident.

The counter drum is driven by an endless screw acting on a toothed edge cut in the drum, the axis of the endless screw terminating in a pinion which is acted on by a bevel wheel attached to the head of the Declination micrometer screw. By long use the screw and fine teeth became worn, so that a slight accidental pressure on the counter-head was sufficient to cause it to slip. The "arbitrary changes" recorded in the published Tables of Nadir appear to be entirely due to the accidental slipping of the counter drum, and the observers have persevered in the same nominal settings of the Declination micrometer. On 1874, November 10, the fine teeth had become so worn at the point corresponding with $10^{r}$ that the counting drum was shifted forty divisions forward, so that the screw might work in teeth that were less worn. From this date till the end of 1879 the observers systematically observed the Nadir at $50^{r}$, but there

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are frequent cases of slipping of the counter (or arbitrary changes of a number of whole revolutions). These are all recorded in the following Table IX. Thus the "arbitrary changes" define a series of epochs, through each of which the Nadir readings were made systematically at nearly the same readings, and all systematically differing from the Nadir readings of the adjoining epochs.

To complete our knowledge of the systematic errors of the N.P.D.'s in each of these epochs (as distinguished from the accidental errors already described) we have only to ascertain the part of the screw at which the determinations of run were made. In observing the run an undeviating routine has been followed, and the great mass of the observations have been made with the lower reading about $5^{\mathrm{r}}{\cdot}2$ to $5^{\mathrm{r}}{\cdot}4$. The general distribution may be judged of from the Table for 1872–3, at p. 81, though after that the range is still more restricted.

In Table IX. the mean lower reading of the run is given for each of the epochs.

TABLE IX.

| 1<br>Rotation Number. | 2<br>Limiting Dates of Epoch. | 3<br>Mean Nadir Reading. | 4<br>Lower Reading for Runs | 5<br>Range of Errors. | 6<br>Mean Error. | 7<br>Systematic Error. |
|---|---|---|---|---|---|---|
| | | r | r | ″ ″ | ″ | ″ |
| I. | 1871, Jan. 1<br>1872, May 12 | 5·3 | 5·3 | −0·59 to +0·41 | ±0·31 | +0·24 |
| II. | 1872, May 13<br>1873, May 28 | 6·2 | 5·3 | −0·90 to +0·10 | ±0·20 | −0·07 |
| III. | 1873, May 29<br>1875, Jan. 31 | 6·7 | 5·3 | −0·95 to −0·05 | ±0·22 | −0·12 |
| IV. | 1875, Feb. 1<br>1875, Feb. 15 | 9·1 | 5·3 | −0·81 to +0·19 | ±0·19 | +0·02 |
| V. | 1875, Feb. 16<br>1875, Mar. 10 | 6·4 | 5·3 | −0·92 to +0·08 | ±0·20 | −0·09 |
| VI. | 1875, Mar. 11<br>1875, Apr. 4 | 6·9 | 5·3 | −0·96 to +0·04 | ±0·22 | −0·13 |
| VII. | 1875, Apr. 5<br>1875, May 10 | 7·4 | 5·3 | −0·98 to +0·02 | ±0·24 | −0·15 |
| VIII. | 1875, May 11<br>1875, May 12 | 9·5 | 5·3 | −0·75 to +0·25 | ±0·21 | +0·08 |
| IX. | 1875, May 13<br>1875, July 20 | 7·0 | 5·3 | −0·97 to +0·03 | ±0·23 | −0·14 |
| X. | 1875, July 21<br>1876, Feb. 20 | 7·6 | 5·3 | −0·99 to +0·01 | ±0·24 | −0·16 |
| XI. | 1876, Feb. 3<br>1876, Mar. 22 | 8·6 | 5·3 | −0·90 to +0·10 | ±0·20 | −0·07 |

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| 1<br>Rotation Number. | 2<br>Limiting Dates of Epoch. | 3<br>Mean Nadir Reading. | 4<br>Lower Reading for Runs. | 5<br>Range of Errors. | 6<br>Mean Error. | 7<br>Systematic Error. |
|---|---|---|---|---|---|---|
| | | r | r | ″ ″ | ″ | ″ |
| XII. | 1876, Mar. 23<br>1876, Oct. 13 | 6·7 | 5·3 | −0·95 to +0·05 | ±0·22 | −0·12 |
| XIII.* | 1876, Oct. 16<br>1878, Dec. 31 | 7·2 | 5·3 | −0·99 to +0·01 | ±0·24 | −0·16 |

In the preceding Table

Column 1 is a rotation number adopted for convenience of reference.

„ 2 gives the range of date in the epoch during which the Nadir readings were made at practically the same parts of the microscope screws, for reasons above stated.

„ 3 gives the mean Nadir readings during the corresponding epoch.

„ 4 gives the mean lower reading for runs.

„ 5 gives the range of error when computed for each $\frac{1}{10}$ of a revolution from $5^{\mathrm{r}}{\cdot}0$ to $10^{\mathrm{r}}{\cdot}0$ from Table V., p. 86, by the method and formulæ on page 87, with the arguments N from Column 3, and R from Column 4 of Table IX.

„ 6 gives the *mean error* (or the square root of the mean of the squares of the errors) computed for each $\frac{1}{10}$ of a revolution from $5^{\mathrm{r}}{\cdot}0$ to $10^{\mathrm{r}}{\cdot}0$.

„ 7 gives the systematic error of the zone, computed on the assumption that the readings fall as often at $5^{\mathrm{r}}{\cdot}0$ as at 5·1, as at 5·2, &c. . . . $10^{\mathrm{r}}{\cdot}0$—in other words, the algebraic mean of the errors computed for each $\frac{1}{10}$ of a revolution.

The most important facts to be deduced from this Table are:

*First.* That whilst the wear of the screws produces accidental errors which range from −0″·99 to +0″·41 (see Column 5), the *mean error* of any N.P.D. taken by chance (as in the comparison of two Catalogues) ranges from ±0″·19 to ±0″·31 (see Column 6).

*Second.* That excepting the two brief epochs IV. and VIII. there is a *systematic difference* of nearly 0″·4 between the N.P.D. for Epoch I. and the rest of the Catalogue.

Mr. Stone appeals to a comparison between the N.P.D.'s of

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* Except on 1876, Oct. 20, when the counter was displaced one revolution, and readjusted before the work of next night. The counter seems to have had a frequent tendency to slip, but during Epoch XIII. it seems to have been always carefully replaced to the same reading for the same revolution of the screw.

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the Cape 1860 and 1880 Catalogues as a proof of the freedom of the latter from errors due to wear of the screws. His selection of the stars rests on a very clear and intelligible basis, and his appeal to such evidence seems at first sight a perfectly fair one.

But it is necessary in discussing so nice a question to take the most minute precautions in arriving at the preliminary data, and here it does not appear that Mr. Stone has stated in sufficient detail the facts which have led to the assumption on which his argument is based.

Mr. Stone quotes "the three independent sources of error" which enter into the discordances, besides the hypothetical errors which may be due to errors of the screws.

These are in his own words the following :—

> (1) There will be the error of the N.P.D. given in the Cape Catalogue 1860; I shall certainly not over-estimate the probable error due to this cause by taking
>
> $$e_{1860} = 0''{\cdot}1.$$
>
> (2) There will be the combined effects of error in the precession constant, and adopted proper motion for 20 years. As this cannot be less than $0''{\cdot}1$, and will be about one-sixth of that due to the relative errors of Bradley's N.P.D. and the recent Greenwich observations, I cannot over-estimate the probable error by taking
>
> $$e_m = 0''{\cdot}2.$$
>
> (3) There will be the probable error of the N.P.D. of the Cape Catalogue of 1880. As there are generally only three or four observations employed in fixing the N.P.D., the probable error, independently of any possible error due to screw wear, will be about
>
> $$e_{1880} = 0''{\cdot}25.$$

Now, as our object is to determine the existence or amount of a small hypothetical error from discordances which include three other independent sources of error, it is obvious that very small errors in our estimates of each of the three independent sources of error may entirely vitiate the result. I propose therefore to re-examine Mr. Stone's assumptions, as he has not stated the grounds on which they have been arrived at.

It must be remembered that the two Catalogues in question rest on observations made with the same instrument, in the same situation, with the same division errors, the same thermometers similarly exposed, and reduced with the same refraction tables. Therefore coincidence in the results is no more proof of absolute accuracy than any similar continuous series of observations with the same instrument in the same year. We may therefore obtain data to arrive at a sufficiently exact estimate of errors (1) and (3) from the accordance *inter se* of the results of N.P.D. for one or more stars observed in a single year. I have therefore selected twelve stars which have been the most frequently observed both in 1856 and 1860, and have deduced the *mean error* of a single observation in N.P.D., and find it to be

$$r_{1860} = \pm 0''{\cdot}7144.$$

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For the 1880 Catalogue I have selected a number of frequently observed stars from the published ledgers, and deduced the mean error of a single observation

$$r_{1880} = \pm 0''\cdot 6214,$$

and these mean errors do not differ very materially from the probable errors of Mr. Stone's sources of discordance (1) and (3).*

But I am quite at a loss to understand the meaning of Mr. Stone's statement (above quoted) relative to his source of error (2). Does Mr. Stone imply that the probable error of the annual variation of stars, such as he has selected (which have been observed by Bradley, Piazzi, and at every observatory in the world since), amounts to ±0″·01 (*i.e.* $\frac{1}{20}$ of 0″·2), and that of all these observations the only reliable data for determining the annual variation are the observations of Bradley and the modern Greenwich observations? I do not think that either one or other of these opinions will receive the support of astronomers.

It fortunately becomes unnecessary to discuss these points here by simply referring Mr. Stone to Professor Lewis Boss's work on the Declination of the fixed stars; where, in the final Catalogue, there is a column which gives for each star the "probable error of annual variation," not as *adopted* by Professor Boss, but as rigorously deduced by him from the data at disposal.

Of Mr. Stone's 93 stars, 53 occur in Professor Boss's Catalogue, and the mean of the probable errors for the annual variations of these 53 stars is ±0″·0031, so that Mr. Stone's estimate appears to be fully three times too great.

The *mean error* corresponding to a probable error of ±0″·0031 is ±0″·00458, which for twenty years corresponds with

$$r_m = \pm 0''\cdot 091,$$

where $r_m$ is the *mean error* of the annual variation in twenty years.

To find, from the discordances between the 1860 and 1880 Catalogues, the mean error of a star's place due to errors of the screw, we have then the following equation from each of Mr. Stone's stars:—

$$(1860 \text{ Cat.} - 1880 \text{ Cat.})^2 = \frac{r^2_{1860}}{n_{1860}} + \frac{r^2_{1880}}{n_{1880}} + r^2_m + y^2,$$

where $r_{1860}$, $r_{1880}$, $r_m$ have the values already found, and where the mean value of $y$ will be found from the mean of the equations.

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* The probable errors (1) and (3) become for the mean number of observations (33 and 34 respectively), ±0″·083 and ±0″·21, instead of ±0″·1 and ±0″·25, as adopted by Mr. Stone.

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This value of $y$ should agree precisely with that of Column 6 of Table IX. (viz. about $\pm 0''\cdot 23$) provided—

($\alpha$) That the Circle screws were absolutely perfect during the period 1855–60.

($\beta$) That each star of Mr. Stone's list was observed throughout the whole of the observations upon which its place in the 1880 Catalogue depends, with the same readings of the micrometer screw. (If this is not assumed, then in each equation we must write $\dfrac{y^2}{n_{1880}}$ for $y^2$).

Now, as a matter of fact, neither of these two conditions is fulfilled. It has already been shown, in my original paper, that during a similar period the screws, which were new and practically errorless in 1880, had acquired decidedly serious errors by wear in 1884. It is, therefore, reasonable to argue that similar screws of similar material and made by the same makers would also acquire similar errors from constant use between 1855 and 1860. The partial wear of the screws in the period 1855–56 would therefore tend to diminish the discordance between the 1860 and 1880 Catalogue, and hence also the value of $y$.

The second condition is not fulfilled because the stars selected for this comparison by Mr. Stone (for other and sufficient reasons) are not zone stars, but clock stars, the Declinations of which have been observed for the 1880 Catalogue at miscellaneous epochs (epoch being here understood in the sense of Table IX.), and consequently in many cases the same star has been observed with different Nadir points and different readings of the micrometer screw. In this way the effect of errors of the screws is to some extent eliminated, and hence the resulting value of $y$ will be diminished.

Thus, because the conditions ($\alpha$) and ($\beta$) are not *fulfilled*, the *mean* value of $y$, computed from the discordances of the two Catalogues, should not amount to the full theoretical amount of Table IX. Column 6—that is, it should be less than $\pm 0''\cdot 23$.

We find accordingly from the sum of the equations—

$$93\, y^2 = 3\cdot 0051,$$

and thence

$$y = \pm 0''\cdot 18.$$

Thus the very stars which Mr. Stone has chosen, when fairly and properly discussed, prove the existence of errors which correspond in effect with those deduced in my original paper from quite different data.

Mr. Stone contends that if $x$ (the portion of the *probable error* in a star's place which is due to errors of the screws) did not amount to $\pm 0''\cdot 5$ (in other words, if $y$ in the present dis-

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cussion did not amount to $\pm 0''\cdot 74$), then my paper should not have been written.

I am sorry that Mr. Stone thinks so.

When it is remembered that, from the form of the curve of error which such a screw acquires from the process of wear, nearly two-thirds of the part of the screw in use must have very small errors, the errors of the remaining one-third of the screw must be very large to produce a computed *mean* error for the whole screw of $\pm 0''\cdot 74$. Some of the absolute errors would in that case amount to nearly $3''$.

Does Mr. Stone contend that all errors under such an amount should be passed without notice or mention? I do not think so; and I feel sure that when Mr. Stone further considers the matter he will not think so.

I hold rather that in fundamental observations we have such numerous sources of error which almost elude our search, that we cannot afford to dispense with the rigid correction of any error that is capable of being so easily and certainly determined as the errors of the screws. It is because I have felt this, and because, in special cases, it will be useful to apply corrections for these screw errors to the 1880 Catalogue, and because it was important that further proof should be given to astronomers of the necessity for rigid determination of the errors of the microscope screws from time to time, that my original paper was written. These are surely sufficient reasons even if the resulting errors had been much smaller than they have proved to be.

But Mr. Stone contends that the corrections which were applicable in 1877 would not be applicable at another time, not because the wear of the screws might be sensibly different in 1871 and 1877, but "from changes in the adjustment of the index from which the revolutions are taken."

It is, however, easy to show that no such changes occurred in the Cape Circle till 1879. The "drums," by which the fractions of a revolution are read, are not attached by mere friction to the screws as in some transit circles, but are squared on, there being a square hole in the drum-head fitting a slightly tapering square on the end of the axis of the screw; and the fit is so nice that the drum will only go home when one marked face of the tapered square is placed against one marked surface of the square in the drum. Thus no change of a fraction of a revolution in the readings can be made.

The bearings of the screws also are immovable, being hollow cones bored in projections cast in one flow with the gun-metal ring to which all the microscopes are attached.

The only way, therefore, in which a real change of a whole revolution in any particular microscope could be made would be by changing the comb one revolution; and to restore the similarity of the numerical readings of the six microscopes, the object-glass of the microscope in question would have to be shifted in a plane at right angles to the microscope axis in a direction parallel to

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the screw. The microscopes have no tubes in the ordinary sense, the mounting of each objective being attached to a piece of brass, which is bolted to the face of the pier, the tubes being simply holes in the pier.

The only means of giving side adjustment to the object-glass is by widening the holes in the flanges, through which the screws pass, by which the objective mountings are screwed to the piece of brass in question.

I do not think that the holes in the flanges of any of the object-glasses are sufficiently widened to allow so large a shift as $\pm 1^r$; and besides, we have it on the authority of Mr. Stone (Introduction to Cape Observations, 1875, p. xi.) that "the effective range of the screws in the Cape microscopes does not extend, at most, over more than six revolutions;" therefore, such a change as one revolution could not be made.

The changes that may have been made from time to time in the relative position of the object-glasses have thus been limited to quantities corresponding with a fraction of a revolution, and as the object of any such changes should have been (and in Mr. Stone's experienced hands would certainly have been) merely to render the readings of the microscopes as nearly the same as possible, there would have been no object in attempting to make larger changes. Such small changes can readily be allowed for by taking for argument in the table of screw corrections not the reading of microscope A only, but the mean of the readings of the six microscopes, and this has been done throughout the present discussion.

Mr. Stone (Introduction to Cape Observations, 1875, p. xi.) quotes observations of the Nadir on 1875, November 22, by three observers, of which the means are the following:—

*Reading of* A.

| | $0^r$ | $2^r\cdot0$ |
|---|---|---|
| Nadir | 180° 7′ 26″·62 | 26″·29, |

and he adds, "it appears from observations made in the present year (1877), and carried systematically over the whole range of the screws, that the difference above indicated is real, but is about as large as the greatest difference obtained throughout the range of the screws employed in the observations."

Why does Mr. Stone not quote those observations of 1877 which prove the reality of the above difference, and which also appear to me to prove differences from two to three times as great (see Table V.), even when negative readings are excluded? Indeed, the above observations seemed to me at first to prove that from the comparatively small discordance (0″·33) between the Nadirs of $0^r$ and $2^r\cdot0$ the screw errors really were different and smaller in 1875 than in 1877. According to our Table V., if the readings were made at $0^r$ and $2^r\cdot0$ (*i.e.* at $5^r\cdot0+7^r\cdot0$) the screw errors should have produced a difference of about 1″ in the two Nadirs.

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But on turning to the original observations I find that the readings of micrometer A were not $0^r$ and $2^r\cdot 0$, but $0^r\cdot 7$ and $2^r\cdot 7$, the mean of the six microscopes being $0^r\cdot 56$ and $2^r\cdot 56$. The lower reading of microscope A for the run was also $0^r\cdot 7$, or for the mean of the six microscopes, $0^r\cdot 56$.

With these means as arguments we can correct the Nadir by our Tables V. and VI.; the corrections are respectively $-0''\cdot 20$ and $+0''\cdot 21$, so that the corrected Nadirs become

*Microscope Reading.*

| | $0^r\cdot 56$ | $2^r\cdot 56$ |
|---|---|---|
| Nadir | 180° 7′ 26″·42 | 26″·50, |

an agreement which is sufficiently precise.

But it may be fairly argued that during the period 1871–77 a very large number of observations was made, and therefore the screw errors would change considerably by wear between these dates. There is no doubt that this must be so, but probably not to the extent that may at first be supposed.

From a discussion of the runs observed at different times, and over different parts of the screw, during the period 1880–1884 (the details of which will be published in the Cape Observations), I find that the wear does not change proportionally with the number of observations, but is expressed for an epoch between 1880 and 1885 by $ak$, where $k$ is the error of the screw produced by wear after N observations (N being actually about 17,000 when the screw errors were determined in 1884), and

$$a = 1\cdot 57\frac{n}{N} - 0\cdot 57\left(\frac{n}{N}\right)^2.$$

where $n$ is the number of observations made since 1880 at the epoch for which the error is required.

That is to say, the wear is much more rapid at first than afterwards; for if the expression were true the wear would cease in eight years, and afterwards diminish! The formula is of course merely used as a sufficiently approximate form of interpolation.

We may also arrive at a like conclusion by other facts.

The second set of screws, mounted in 1880, are of the same material and made by the same artists as the first set. We may probably conclude, therefore, that the law of wear of both sets would be nearly alike.

The curve of errors of the new screws (as determined by careful measurement with the apparatus described and figured at pp. 64–65) is given in the following diagram (fig. 8); these errors result from four years' wear, and are indicated by a continuous curve.

The curve of the errors of the old screws, resulting from twenty-one years' wear, is indicated by a dotted curve in the same diagram.

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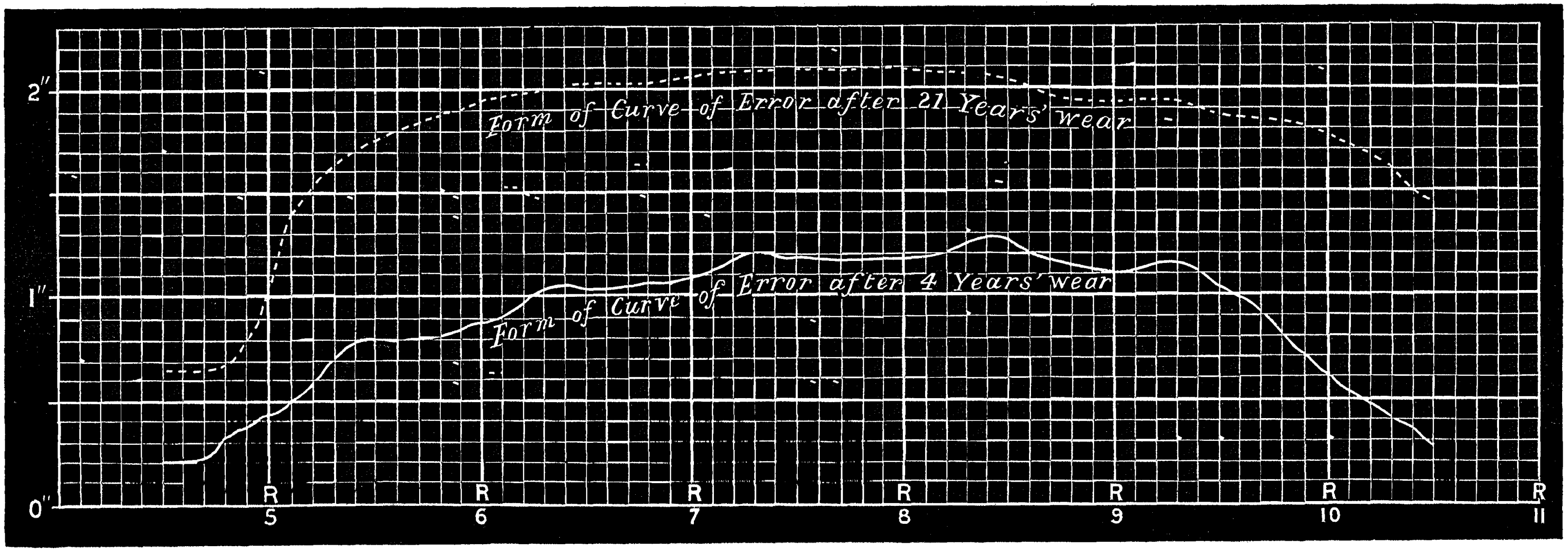

Fig. 8.—Curves showing errors of screws after twenty-one, and four years' wear.

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It becomes obvious that the wear has been much less rapid during the last seventeen years than in the first four years of the screw's work, else the two curves would differ much more widely. This is probably because the deeply-cut male and female threads do not at first bear along the whole of the faces, but gradually, by mutual wear, attain to more complete contact of their bearing surfaces.

The discussion of a comparison between Right Ascensions of the Cape Catalogues for 1850 and 1880 appears to be somewhat irrelevant to a paper on screw wear; I therefore defer reply to Mr. Stone's remarks on this subject till a more fitting opportunity.

## *The Velocity of Meteors.* By W. F. Denning.

At the December meeting of the Royal Astronomical Society, and again at the May meeting, certain remarks were made as to the velocity of meteors and the enormous speed necessary to explain the occurrence of fixed, long-enduring radiant points. I wish to say that no such velocities are observed, and that it is impossible the fixed radiants can be explained by this means. I quite agree with the remarks that emanated from Col. Tupman as to the difficulty of accounting for these long-continued showers, and also that the very great velocities indicated by Mr. Proctor have never been recognised. The meteors falling from the stationary radiants, referred to in my paper read at the December meeting, appear to be of ordinary character, and their motions cannot differ essentially from the parabolic velocities exhibited by the planetary streams.

Mr. Ranyard, at the May meeting, asked for information as to whether the speed from any of the assumed fixed radiants had been determined from multiple observations of bolides and large meteors. The velocity has certainly been computed in several instances, and they accord with that of the usual planetary meteors. Of course, there is rather a wide limit of error in estimating the exact durations of flight, the intervals being so transient in most cases; but it is impossible that the meteors can travel at a rate anything approaching that suggested as the only explanation of the long enduring showers.

Let us take the instance of the display from near ε *Persei* at 61°·8+36°·8, No. IV. of my list in the *Monthly Notices* for Dec. 1884, p. 101, which I regard as one of the very best cases of stationary radiation. A shooting star of about the 3rd mag. was doubly observed from this shower, at York and Oxford, at $11^h\ 28\frac{1}{2}^m$ on Aug. 10, 1872. The estimated duration was 0·5 second, and the actual length of the path traversed in the atmosphere was 20·4 miles, so that the resulting velocity was about forty-one miles per second. The radiant point was at